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CONTENTS:

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PROCEEDINGS OF THE AMERICAN PHYSIOLOGICAL SOCIETY.

The American Physiological Society held its Seventh Annual Meeting in Baltimore, Md., December 27th and 28th, 1894. The mornings were devoted to the reading of papers, and the afternoons to demonstrations and to visiting the laboratories of Johns Hopkins University. The success of the meeting was largely due to the hospitality of Johns Hopkins University, the University Club and friends of the Society residing in Baltimore.

ELECTION OF NEW MEMBERS.

DR. A. C. ABBOT, First Assistant at the Laboratory of Hygiene, University of Pennsylvania.

DR. G. CARL HUBER, Assistant Professor of Histology and Embryology at the University of Michigan.

DR. P. A. LEVENE, of New York City.

DR. FRANZ PFAFF, of Boston.

ELECTION OF THE COUNCIL FOR 1894-95.

H. P. BOWDITCH, *President.*
R. H. CHITTENDEN.
W. H. HOWELL.
F. S. LEE, *Secretary and Treasurer.*
W. P. LOMBARD.

Reading of Papers and Demonstrations by Invited Guests and Members of the Society.

On the Occurrence of Diæthyl Sulphide in the Urine of the Dog, with a Demonstration of Reaction for the Detection of Alkylsulphides of the Series $(C_nH_{2n+1})_2S$. J. J. ABEL.

Dr. Abel demonstrated in a series of reactions, many of them new, that the volatile, odoriferous compound that is liberated when dog's urine is treated with alkalies is ethyl sulphide $(C_2H_5)_2S$, and also that the organic sulphides of the series $(C_nH_{2n+1})_2S$ may readily be detected, wherever found, with the help of his reactions.

On the Use of the Trichloride of Acetonic Acid as Anæsthetic for the Laboratory, with Some

Account of its Fate. J. J. Abel and T. B. Aldrich.

Drs. Abel and Aldrich gave an experimental demonstration of the use of the solid trichloride of acetonic acid of Willgerodt, the so-called acetone chloroform, as an anæsthetic for the laboratory, with an account of its physiological action and of its fate, from a chemical point of view, in the economy.

Demonstration of Instances of Experimental Cachexia Tyreopriva in Dogs. J. J. Abel and A. C. Crawford.

Drs. Abel and Crawford showed a number of dogs whose thyroid glands had been removed. They also gave an account of their results in treating the diseased conditions thus induced, and outlined the methods and aims of a research on the functions of the thyroid gland.

Equilibrium in the Crustacea. G. P. Clark. (Introduced by F. S. Lee.)

Dr. Clark stated that he had studied two kinds of crabs, the 'Fiddler,' *Gelasimus pugilator* (Latr.), and the 'Lady,' *Platyonichus ocellatus* (Latr.). The former is an active runner, the latter an active swimmer. The movable eyestalks show marked compensating movements when the body is inclined. The compensating positions are maintained without reaction so long as the inclination of the body continues. No compensating movements accompany turning around the vertical axis. The otocysts contain no otoliths. Removal of both antennules, inclusive of the otocysts, caused no abnormal position of the body and no forced movements, but was followed by a tendency of the 'Fiddler' crab when attempting to run, and of the 'Lady' crab when attempting to swim, to roll over on to the back. A similar tendency has been observed by others in the crayfish and dogfish after removal of the otoliths. Removal of both antennules was followed by no abnormal position of eyestalks, but by marked diminution of their compensating movements. Removal of otoliths from both ears of a dogfish is reported to be without effect on position of eyeballs, but to cause a loss of the *maintenance* of compensation which is observed in those rotations which involve inclination of the body. Compensating eye movements in the crab occur only in those planes in which in the dogfish the compensation is maintained, and loss of corresponding structures in these animals tends to destroy compensation in the one and the maintenance of compensation in the other. In many cases it was found that a small amount of compensation remained after the 'Fiddler' crab had lost both antennules; if eyes were then covered with a thick black mixture it was completely stopped.

Galen's Technical Treatise upon Practical Anatomy and Experimental Physiology. J. G. Curtis.

Dr. Curtis spoke upon Galen's technical treatise on practical anatomy and experimental physiology, usually cited as '*De anatomicis administrationibus.*'

This was written between A. D. 150 and 200, and is the earliest existing technical treatise upon these subjects.

The Greek text of Books I. to VIII., and of part of Book IX., is extant in print, and also Latin translations of the same.

The rest of the work, viz., the latter part of Book IX., and Books X. to XV., is inedited, and is contained only in two MSS. of an Arabic version of the 9th century, attributed to Honain Ibn Ishak or to his nephew Hobaich.

One of these two MSS. is at the Bodleian Library at Oxford. By the kindness of the authorities, Books IX. to XV. of this MS. have been photographed for Dr. Curtis, who is also, through the good offices of the late Dr. Greenhill, of Hastings, England, in possession of an inedited MS. sketch of a translation of these books into French, by the late M. Gustave Dugat.

Dr. Curtis proposes to edit, and to have published, a translation into English of the entire treatise, the Greek portion to be translated by himself, and the inedited Arabic portion by a collaborator not yet named.

This English translation will be the first complete edition of the 'epoch-making' Galenic work in question published in any language since the invention of printing.

The Normal Defect of Vision in the Fovea.

MRS. C. L. FRANKLIN. (Introduced by H. P. BOWDITCH.)

König's announcement, in May, 1894, that the relative absorption by the visual purple of the different portions of the spectrum is in very close coincidence with the relative brightness of the different portions of the spectrum, (1) for the totally color-blind, and (2) for the normal eye for faint light after adaptation (with the obvious inference therefrom that the vision of the totally color-blind and that of the normal eye in a faint light was conditioned upon the presence of the visual purple in the retina), made necessary some assumption to take account of the fact that in the fovea, which is the portion of the retina where vision is most acute, no visual purple has hitherto been found. Two assumptions were possible, either that the cones (and hence the fovea) do contain visual purple, but that it is here of such an extremely decomposable character that it can never, no matter what precautions are used, be detected objectively; or, that vision does actually not take place in the fovea under the above circumstances (that is, for the totally color-blind and for the normal eye at such intensities as are visible only after adaptation). As I had already made the prediction that total color-blindness consists in a non-development of the *cones* of the retina (*Ztsch. f. Psych. u. Phys. der Sinnesorgane*, Bd. IV.) and also that the adaptation which renders vision possible after twenty minutes in a faint light is conditioned by the growth of the visual purple (*Mind*, N. S., III., p. 103), both predictions being naturally suggested by my theory of light-sensation, I was most anxious to put the latter assumption to the test. I therefore undertook to determine, in the dark rooms of Prof. König's laboratory, the threshold for light-sensation for different parts of the retina and for different kinds of monochromatic light.

The blindness of the fovea for faint light did not at once reveal itself; the act of fixation means holding the eye so that an image falls on the part of the retina best adapted for seeing it, and hence it would involve keeping the image *out of* the fovea in a faint light, if the fovea were really blind in a faint light. But after the total disappearance of the small bright object looked at had several times occurred by accident, it became possible to execute the motion of the eye necessary to secure it at pleasure. It was then found that the simple devices of presenting a group of small bright objects to the eye of the observer was sufficient to demonstrate the 'normal night-blindness of the fovea' (as it may best be called) without any difficulty; one or the other of them is sure to fall into the dark hole of the fovea by accident. It was only by means of this arrangement of a number of small bright spots that the total blindness of the totally color-blind boy in the fovea could be detected; he had, of course, learned *not* to use his fovea in fixation. Professor König then proceeded to demonstrate the total blindness in the fovea of the normal eye to blue of about 470.*

[These experiments upon the normal eye were exhibited.]—It was shown that König's proof that the pigment-epithelium

*Professor v. Kries is said by Professor Gad to have shown that the experiments in question do not establish the *blue*-blindness of the fovea (*Berichte der Naturforschenden Gesellschaft zu Freiburg*, IX., 2. S. 61). I have not yet had access to this criticism.

is the only layer of the retina which is affected by red, yellow and green light is not wholly conclusive. The interpretation of the new facts and their bearing upon several theories of light sensation were discussed.

[This paper will appear in full in *The Psychological Review* for March, 1895.]

The Influence of low Percentages of Alcohol upon the Growth of Yeast. C. F. Hodge.

The influence of decomposition products upon cellular metabolism is a question of wide physiological interest and has increased in significance since the advancement of recent theories regarding autointoxication. Do the decomposition substances of initial activity stimulate the cells to more active metabolism? Aside from the general question of the physiological effect of alcohol upon cellular processes, the influence of alcohol upon the cell which produces it would seem to be one of the best instances upon which to test the theory of autointoxication. Yeast can grow in a saccharine solution until by the decomposition of sugar it has brought the alcohol content of the liquid up to 14%. With a greater amount of alcohol no growth is possible. Flügge also states that at 12% growth is hindered. Experiments were made with exceedingly attenuated pure cultures in large amounts of nutrient solution, containing from .01%, .1% up to 14 %. Counts were made as often as possible during the first three days. The general result up to the present is that yeast grows nearly twice as fast in pure solution as in 1% alcohol. An average of nine experiments thus far give the following figures representing proportional growth in the various cultures.

Growth in: 0%, 1%, 2%, 3%, 4%, 5%, alcohol.
77, 45, 16, 1.5, 0.3, 0.11.

Beyond 5% no growth appreciable by the method employed occurred within the three days. In cultures containing 0.1% and 0.01% growth was considerably less than in the normal solution; but it is desirable to experiment further before giving the figures. As yet no evidence in favor of autointoxication theories has been obtained.

A Means of Recording Daily Activity of Animals and the Influence upon it of Food and Alcohol. C. C. Stewart. (Introduced by C. F. Hodge.)

Thus far the animals experimented on have been rats, mice and squirrels. They are kept in circular, easily rotated cages, so arranged that any motion of the animal rotates the cage, and by means of a tambour or levers this motion of the cage is recorded upon kymograph paper kept moving night and day. An electromagnetic circuit with a clock marks hours and minutes. We thus have the manner in which an animal divides his time between rest and activity recorded by himself. Rats and mice divide their days into about 12 hours rest and 12 hours intermittent work during the night. During the work period, short intervals of activity, rarely exceeding an hour, are interrupted by almost equal periods of rest. The squirrel, in winter, works almost continuously for from twenty minutes to two hours early in the morning, with sometimes a short interval of activity late in the evening, and rests nearly 22 hours in the day.

Food has a most marked influence upon diurnal activity. In general the richer the diet in proteid, the greater the activity. Fat has the opposite effect, reducing the activity of mice from 6 to 8 hours' actual work to a few minutes a day. To test the influence of alcohol on spontaneous activity, rats kept on dry corn were given instead of water alcohol of from 5% to 60%. During 50 days of his treatment, no uniform effect of the alcohol could be demonstrated. All normal animals experimented on tended to work more minutes per day, when barometric pressure was high, and this must be taken into careful account in estimating the effect of any condition upon daily activity.

A Study of the Operative Treatment for Loss of Nerve Substance in Peripheral Nerves. G. CARL HUBER. (Introduced by W. P. LOMBARD.)

The report covered the results obtained in 50 experiments on dogs, in which the various methods that might be employed in the surgical treatment of divided peripheral nerves, where there is loss of nerve substance to the extent that an ordinary suture cannot be made, were tried. Segments varying in length from 5–8 cm were removed from the ulnar and sciatic nerves of the dogs. In 26 experiments a portion of another nerve (usually the sciatic of a cat) was implanted between the resected ends of the nerve operated upon, and retained in place by means of sutures; in 8 experiments the resected ends were united by means of decalcified bone tubes; in 7 they were united with a number of catgut threads; a flap from the peripheral end of the central stump was made in 7 experiments; and grafting the central end of the peripheral portion of a resected nerve to an accompanying nerve trunk was tried twice. After carefully closing the wounds, the animals were allowed to live for periods varying from 2 to 182 days; before killing the animals the nerves operated upon were tested as to their conductivity; they were then removed and prepared for histological examination.

1. In all experiments the peripheral portion of the divided nerve degenerated, as also ½ cm. of the distal end of the central stump.

2. Regeneration was obtained after implantation of a nerve segment, tubular suture and suture *à distance* with catgut threads.

3. Regeneration was from the central end, buds given off from the central axis cylinders growing toward the periphery.

4. The implanted substance serves only as a guide to the down growing axis.

5. Regeneration takes place most rapidly (120 to 130 days in dogs) after implantation of a nerve segment.

Demonstration of a New Gas Pump for the Extraction of Blood-Gases. G. T. KEMP.

Dr. Kemp exhibited and explained the action of a new form of gas-pump. This pump is, except for slight modifications, a combination of the Sprengel pump with the Neeson and Bessel-Hagen additions to the Toepler pump. The large bulb is used in accordance with a suggestion of Pflüger and is about the size of those in the large pumps used in the laboratory at Bonn. The pump is made in two halves for ease of transportation. The vacuum space on each side of the bulb prevents the mercury from spitting back into the bulb, during the first few lowerings of the reservoir, as occurs in the Neeson-Bessel-Hagen-Toepler pump. The advantage of this form of pump over all patterns which have a 3-way stopcock at the top of the bulb, is that there is no danger of smashing the stopcock from the impact of the mercury, and the pump can be worked very much faster. No precaution has to be taken against raising the reservoir bulb too rapidly.

The Sprengel attachment can be made to work either separately or together with the other part of the pump.

There is no stopcock which is not completely under mercury seal, so that leakage is out of the question.

The essential requisite of such a pump is to extract all the oxygen as soon as possible, certainly before the blood clots, and to keep the tension in the blood bulb from rising above 20 mm. of mercury, as this prevents the complete disassociation of the oxygen from the oxyhaemoglobin. When blood is drawn into the vacuum the oxygen is given off very rapidly, in a 'puff,' so to speak, and the carbon dioxide is given off more slowly and regularly. By having a large Hg bulb which can be filled and emptied rapidly, the exhaustion can easily be main-

tained so as to keep the tension below 20 mm. of mercury, and after the oxygen is set free the Sprengel part is left working alone, and that carries off the CO_2, as it is slowly evolved, without necessitating close attention of the operator or the fatigue of raising and lowering the reservoir bulb of mercury.

Further Experiments Upon Equilibrium in Fishes. F. S. Lee.

Previous work of Dr. Lee has shown that the organs of the sense of equilibrium lie in the ear, the semicircular canals mediating sensations of movements in curves, the otolithic parts sensations of the resting body. Recent experiments prove that the otolithic parts are, moreover, sensory organs for progressive movements, *i. e.*, movements in a straight line. Hence the ear deals with all three groups of equilibrium sensations of which the living body is capable.

Stimulation of the central end of the lateral nerve causes coördinated movements of the fins, analogous to those resulting from stimulation of the acoustic. This indicates that the organs of the lateral line are organs of equilibrium.

All experiments to prove that fishes possess a sense of hearing have so far given only negative results.

Equilibrium in the Ctenophora F. S. Lee.

Dr. Lee reported the results of experiments made under his direction by Mr. J. C. Thompson on the equilibrium phenomena of the Ctenophora. The normal animal exhibits definite positions of rest and definite coördinated movements. After removal of the otolith the resting positions are no longer maintained, and incoördination in movement appears. Forced movements do not result. If the body be cutinto two parts, one with and one without the otolithic organ, the former maintains its equilibrium, he latter doest not. All attempts to demonstrate a sense of hearing failed.

The two following papers, because of the lack of time, were read by title:

On changes of Structure in the Pancreatic Cell corresponding with Functional Change. A. P. Mathews. (Introduced by F. S. Lee.)

On the Existence of Secretory Nerves. A. P. Mathews. (Introduced by F. S. Lee.)

On Cardio-oesophagogeal Movements. S. J. Meltzer.

Dr. Meltzer has shown in a former paper that the outflow of arterial blood from, and the inflow of venous blood to, the thorax produce the cardiac movements which are obtainable from the pleuritic cavity as well from the trachea and the nose. In this paper he described the cardio-oesophageal movements arising from the same cause. He exhibited tracings which he obtained fourteen years ago from his own oesophagus, while studying the mechanism of deglutition. His recent studies were made on curarized dogs. By means of vagus inhibition the beginning and the end of each cardiac cycle were made recognizable. Nearly all the curves have the character of a 'negative pulse' and have no similarity either to a sphygmo- or cardiogram. The constant characteristic undulationse seen at the beginning of each cardiac cycle are due to the movements of the auricle, which are more marked in the posterior mediastinum.

Cortex of the Brain: (a) Localization; (b) Development of. T. W. Mills.

Dr. Mills undertook this research in connection with a study of the psychic development of young animals. It became necessary, however, as a precaution and guide in studying the functional development of cortical centres to make experiments on mature animals. While, during these experiments, most of the commonly accepted localization as set forth by Ferrier was verified in a general way, the results did not all harmonize with those of this investigator. Attention was called to details in the cortical motor localization of the rabbit and pigeon more especially, which were at vari-

ance both positively and negatively with those announced by Ferrier.

There had been found a great difference in the degree of cortical development of mammals not born blind as compared with those born with the eyes unopened; but as the work was not complete the author preferred not to make many very definite statements at the present time. Cortical development and psychic development took place *pari passu*.

The Active Principle of Rhus Toxicodendron and Rhus Venenata. FRANZ PFAFF. (Introduced by H. P. BOWDITCH.)

Dr. Pfaff stated that his experiments had been made with the assistance of S. Sanford Orr. He said that it is the general opinion that *Rh. tox.* and *Rh. ven.* contain a volatile proximate principle, which causes the well-known dermatitis venenata. Maisch's toxicodendric acid has been generally accepted as the active poison. P. and O. could not believe that a very volatile substance is the cause of the trouble, as this would be contrary to the pharmacology of vegetable skin irritants. They isolated Maisch's toxicodendric acid in the form of the barium salt, and found it non-toxic. The same is true of a solution of the free acid in water. As the real active principle they found a non-volatile oil. This oil, when applied to the skin, causes the well-known eruption. Photographs demonstrating the effect of the oil upon the human skin were shown. As preventive treatment P. and O. proposed a thorough washing with water, soap and brush, or, still better, a repeated thorough washing with an alcoholic solution of lead acetate. The oil being soluble in alcohol, and forming a nearly insoluble lead compound in alcohol, is thus best removed from the superficial skin. Further investigations will be undertaken, and an attempt made to classify Maisch's toxicodendric acid and the new poisonous oil, which seems to be of the kind called cardol, obtained from *Anacardium occidentale*. These two oils are, however, not identical.

Inhibition Hypothesis in the Physiology of Respiration. W. T. PORTER.

Dr. Porter said that it is known that transverse division of the spinal cord between the bulb and the phrenic nuclei causes fatal arrest of the respiratory movements of the trunk. If death be prevented for a time by artificial respiration, the reflex powers of the cord gradually increase, and in the course of a few hours they may become so great that pinching the paws, blowing on the skin, suspending the artificial respiration, etc., may cause extended muscular contractions, including contractions of the respiratory muscles.

It is claimed that these contractions of the respiratory muscles after the separation of the cord from the bulb are proof that the respiratory impulse for muscles of the trunk is not derived from respiratory cells in the bulb but originates in the spinal cord. Against this hypothesis of spinal respiration is urged the fatal arrest of the respiration of the trunk caused by separating the bulb from the cord. It is replied that section of the cord stimulates inhibitory fibres in the cord and thus suspends the action of the spinal respiratory cells. This inhibition, it is assumed, usually lasts throughout the period of observation; in some animals, however, after long artificial respiration, it is partially overcome, permitting the respiratory contractions mentioned above.

The doctrine of prolonged inhibition of spinal respiration is easily overthrown by the following experiment. Hemisection of the cord usually arrests the contractions of the diaphragm on the side of the hemisection. (Exceptions are explained by 'crossed respiration.') This arrest is not an inhibition, for the diaphragm on the side of the hemisection begins at once to contract when the opposite phrenic nerve is cut. Hence, hemisection of the cord between the bulb

and the phrenic nuclei does not inhibit the the phrenic cells on the side of the section.

It follows that two hemisections, completely separating the cord from the bulb, do not inhibit the diaphragmatic respiration on their respective sides. The phrenic cells often send out no respiratory impulses after such a section because they receive none from the bulb. The phrenic cells cannot themselves originate respiratory impulses. Hence, the respiratory impulse does not arise in the spinal cord.

Demonstration—Hemisections of the Spinal Cord above the Phrenic Nuclei do not inhibit Thoracic Respiration. W. T. Porter.

Acuteness of Vision in St. Louis Public School Children. W. T. Porter.

The Weight of Dark-haired and Fair-haired Girls. W. T. Porter.

Exhibition of Some New Forms of Galvanometers Suitable for Physiological Use, With Remarks Upon the Same. Prof. H. A. Rowland, at the Physical Laboratory of Johns Hopkins University.

Professor Rowland exhibited two new forms of high resistance galvanometers. One was a modification of the Thompson galvanometer, but less expensive in construction, and possessed a greater delicacy; the other was a modification of the Darsonval galvanometer, and was arranged with the observing telescope on a convenient wall support. It was shown that they were well adapted for laboratory use in Physiological work.

Demonstration of an Apparatus for the Plethysmographic Study of Odors, with Report of Results. T. E. Shields. (Introduced by W. H. Howell.)

Mr. Shields exhibited his apparatus, and gave the following account of its use:

1. It consists of a device for holding the arm firmly in place in the Plethysmograph. Two hard rubber clasps, one fitting the wrist and the other the arm above the elbow, are rigidly connected by two metal rods. The latter of the clasps fits against the Plethysmograph under the rubber membrane, where it is held in place by two other rigidly connected clasps, one against it outside the rubber membrane, and the other against the flange of the Plethysmograph.

2. A device for separating the pulse and vaso-motor curves. A short *wide* tube leads from the Plethysmograph to a vertical glass cylinder in which the water level can be made to register the pressure on the arm. Over the water is an air cushion connected with the tambour by a small tube through a piston movable in the cylinder. The motion of the piston controls the size and pressure of the air cushion. The lever of the tambour is made to move the point of an independently supported pen. A long *narrow* tube leading from the Plethysmograph dips into a test-tube of water swung from a delicate spiral spring. (Method described by Professor H. P. Bowditch.) A vertical thread from the bottom of the test-tube passes under a pulley, thence horizontally over a second pulley, and is held taut by a small weight. On its horizontal part is fastened a thin aluminum plate capable of holding a glass pen at right angle to the thread. The bulb of the pen is independently suspended by a vertical thread. The pendular motion due to the latter in the direction of the horizontal thread is so adjusted as to neutralize the curvilinear motion of the pen arising from the sag in the horizontal thread. The point of the pen may thus be made to describe a straight horizontal line. The resistance to the motion of the water in the *narrow* tube is sufficient to destroy all but vaso-motor effects; pulse effects are, in consequence, only felt through the *wide* tube.

The odors are contained in a series of bottles. The turning of a stopcock, which sends the constant current of air through any particular odor-bottle, at the same time, by an electrical arrangement, marks the in-

stant, and opens the terminal end of the corresponding tube near the subject's nose.

A pneumograph records the respiration. The pulse, vaso-motor and respiratory curves, the signal and time records (in seconds) are all traced in ink on a horizontal kymograph.

Explanation of Natural Immunity. GEORGE M. STERNBERG.

Dr. Sternberg, after a review of the experimental evidence relating to the cause of the natural immunity which exists among animals against parasitic invasion by various pathogenic bacteria and by putrefactive microörganisms, said that the experimental evidence submitted, considered in connection with the extensive literature relating to 'phagocytosis,' leads us to the conclusion that natural immunity is due to a germicidal substance present in the blood serum, which has its origin (chiefly at least) in the leucocytes, and is soluble only in an alkaline medium. And that local infection is usually resisted by an afflux of leucocytes to the point of invasion, but that phagocytosis is a factor of secondary importance in resisting parasitic invasion.

WARREN P. LOMBARD,
UNIVERSITY OF MICHIGAN. *Secretary for 1894.*

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*AN INHERENT ERROR IN THE VIEWS OF GALTON AND WEISMANN ON VARIATION.**

WEISMANN'S name has become so intimately associated with the doctrine of germinal continuity that he is often regarded as its first advocate, although it is an old conception which has found expression in many writings.

Among others I myself stated it in the following words in a book printed in 1883, before the publication of Weismann's first essay on inheritance.

"The ovum, like other cells, is able to reproduce its like, and it not only gives rise, during its development, to the divergent cells of the organism, but also to other cells like itself. The ovarian ova of the offspring are these latter cells or their direct unmodified descendants."

After the appearance of Weismann's essays, and the revival of discussion on the views of Lamarck, I was much surprised to find my book referred to as a Lamarckian treatise, and my reason for quoting this passage now is not to claim priority, but to show that, in 1883, I, like Weismann, attributed inheritance to germinal continuity.

I may take this occasion to say that I still regard inheritance as a corollary or outward expression of the continuity of living matter, although I am less confident than I was in 1883 of the importance of the distinction between somatic and germinal cells. So much for the doctrine of germinal continuity.

Passing now to another topic, we find that the two most prominent writers on inheritance, Wiesmann and Galton, base their views of variation on the assumption that, at each remote generation, the ancestors of a modern organism were innumerable, although a little reflection will show that this assumption is untenable.

Weismann, at least in his earlier and simpler writings, finds the cause of variation in the recombination, by sexual reproduction, of the effects of the diversified influences which acted upon the innumerable protozoic ancestors of each modern metazoon.

If it can be proved that these protozoic ancestors were not innumerable, but very, very few, and that these few were the common ancestors of all the modern metazoa, his position is clearly untenable.

Galton's view of the cause of individual diversity is very similar to Weismann's. He says: "It is not possible that more than one-half of the *varieties* and number of the parental elements, latent or personal, can on the average subsist in the offspring.

* A paper read, by invitation, at the meeting of the Society of Naturalists, in Baltimore, Dec. 27, 1894.

For if every variety contributed its representatives each child would on the average contain, actually or potentially, twice the variety and twice the number of the elements, whatever they may be, that were possessed at the same stage of its life by either of its parents, four times that of any of its grandparents, 1024 times as many as any of its ancestors in the tenth degree and so on."

As he holds that each offspring must therefore get rid, in some way, of one-half the variety transmitted from its ancestors, he finds an explanation of the diversity between individuals in the diversity of the retained halves of their variety.

Each person has two parents and four grandparents; but even in a country like ours, which draws its people from all quarters of the earth, each of the eight grandparents is not always a distinct person; for when the parents are cousins, this number is six, or five, or even four, instead of eight.

Among more primitive people who stay at home generation after generation, and marry within the narrow circle of their neighbors, a person whose ancestors have transgressed none of our social laws may have a minimum ancestry of only four in each generation.

The maximum ancestry and the minimum fixed by our customs are given for ten generations in the two lines below.

2–4–8–16–32–64–128–256–512–1024=2046.

2–4–4–4–4–4–4–4–4–4=38.

Few persons who can trace their ancestry back for ten generations are descended from 1024 distinct persons in that generation, and in all old stable communities of simple folks the number is very much smaller. In the long run the number of ancestors in each generation is determined by the average sexual environment, and it is a small and pretty constant number.

All genealogy bears indirect evidence of this familiar fact which has not been adequately recognized by students of inheritance.

I have made a computation from the history of the people of a small island on our Atlantic coast. They lead a simple life, or have done so in the past, but most of the men have been sailors, and have ranged much farther in search of mates than agricultural people. I have selected three persons whose ancestry is recorded in detail for some seven or eight generations. These three persons have no parents or grandparents of the same name, and they would not be popularly regarded as near relations, although two of their twelve grandparents were cousins. The generations are not quite parallel, and the period covered by eight in one line is covered in the two others by about seven, and it may be put at about $7\frac{1}{2}$ for the three. In $7\frac{1}{2}$ generations the maximum ancestry for one person is 382 or 1146 for three persons.

The names of 452 of them, or nearly half, are recorded, and these 452 named ancestors are not 452 distinct persons, but only 149; many of these in the remoter generations being common ancestors of all three persons in many lines. If the unrecorded ancestors were interrelated in the same way as they would surely be in an old community, the total ancestry of the three persons for $7\frac{1}{2}$ generations would be 378 persons instead of 1146.

Few persons know even the names of all the living descendants of each of their sixty-four ancestors of the sixth generation, and marriage with one of them is a pure chance, depending on the size of the circle of acquaintance and the distance to which ancestors wandered.

If a city like Baltimore, where the strangers to each one of us outnumber our acquaintances a thousand fold, could be quarantined against people from outside for a thousand years, each generation would be much like the present one so far as known

relations are concerned, although at the end of the period the inhabitants would certainly not be descended from the Baltimorians of our day, but from only a very few of them. Most of our lines would be extinct, and the few which survived would include most of the Baltimorians of the year 2900 among their descendants, who, while unconscious of their common origin, would be allied with each other by common descent from their virile and prolific ancestors of the year 1894.

This is proved indirectly but conclusively by genealogical statistics, and while a thousand years are but as yesterday in the history of species, zoölogical considerations furnish evidence that allied animals at two successive geological periods must be related like these successive generations of Baltimorians. Of all the individuals of a species which lived at a given period, very few would have descendants at a later period, and these few would be the common ancestors of all the individuals which represent the stock at the later period.

The extinction of species is a familiar conception. The extinction of the lines of descent from individuals is no less real, and infinitely more significant in the study of inheritance.

As we trace back the ancestral tree it divides into two branches for the parents, and again into four and eight for the grandparents and great-grandparents, and so on for a few generations, but a change soon takes place. The student of family records may be permitted to picture genealogy as a tree whose branches become more and more numerous as we get farther and farther from the starting point; but this cannot be permitted to the zoölogist.

On the contrary, we must admit that, on the average, the number of ancestors in each generation can never be greater than the number of individuals in the average sexual environment. It may be very much less, however, since most of the individuals in each generation must fail to perpetuate their lines to remote descendants.

Now no animal in a state of nature ranges so far as man in search of a mate, and the sexual environment of many plants and animals, such as the fishes in a brook or a pond, or the parasites in the intestine of a mammal, is very narrow. While new blood, no doubt, finds its way in from time to time, this is more than balanced by the extinction of genetic lines. The series of ancestors of each modern organism is long beyond measure, but the number of ancestors in each remote generation can never be very great, though it may be extremely small.

The data of systematic zoölogy also force us to believe that the ancestry of all the individuals of a species has been identical, except for the slight divergence in the most recent part of their history.

The zoölogist must picture the genealogy of a species not as a tree, but as a slender thread, of very few strands, a little frayed out at the near end, but of immeasurable length and so fine that the thickness is as nothing in comparison. The number of strands is fixed by, but is much smaller than, the average sexual environment. If we choose we may picture a fringe of loose ends all along the thread to represent the ancient animals which, having no descendants, are to us as if they had never been. Each of the strands at the near end is important, as a possible line of union between the thread of the past and that of the distant future.

The gist of the whole matter is this, that we must picture this slender thread as common to all the individuals of the species, whose divergence from each other is infinitesimal compared with the ancestry they share in common.

The branches of a human genealogical tree diverge for a few generations by geometrical progression, but we soon find traces of a change, and if the record were long

enough to have any evolutionary significance we should surely find all the members of a species descended from a few remote ancestors, and these few the common ancestors of all. If one metazoon is descended from pre-Cambrian unicellular ancestors, the same unicellular individuals were the common ancestors of all the metazoa, and we may be confident that there were not very many of them in each generation. It is quite possible that they were even so few as a single pair or even one.

There is nothing very novel in all this. Galton has himself devoted an appendix to the mathematical study of the extinction of family names, although he and other writers on inheritance seem to forget it when they assume that the remote ancestors of two persons, A and B, were, like the parents, distinct individuals, and that the offspring must have twice as much ancestry as either parent, and, therefore, twice as much variety, unless there is some way to cancel out half of it at each step.

I called attention to the bearing of this convergence of ancestry on the problem of inheritance in 1883, in words which still seem to be a clear statement, although the views on variation of both Galton and Weismann are based on the unfounded assumption that each sexual act brings together two totally dissimilar sets of factors, instead of factors which are identical in innumerable features for each one in which they differ.

My statement is as follows: "In order to breed together, animals must be closely related; they must belong to the same species or to two closely allied species. Since the individuals which belong to two closely related species are the descendents of a common and not very remote ancestral species, it is clear that almost the whole course of their evolution has been shared by them in common; all their generic characters being inherited from this ancestor. Only the slight differences in minor points which distinguish one species of a genus from another have been acquired since the two diverged, and not even all of these slight differences. * * We know that the duration of even the most persistent species is only an infinitesimal part of the whole history of their evolution, and it is clear that the common characteristics of two allied species must outnumber, thousands of times, the differences between them. It follows that the parents of any possible hybrid must be alike in thousands of features for one in which they differ. * * Crossing simply results in the formation of a germ by the union of a male and a female element derived from two essentially similar parents, with at most only a few secondary and comparatively slight differences, all of which have been recently acquired."

I trust that you will not think me unwarranted in the assertion that due consideration of the substance of this extract might have saved us much unprofitable discussion of the causes of variation, for I hope I have made it clear that these must be sought in the modern world and not in the remote past; that, as I expressed it in 1883, "the *occurrence* of a variation is due to the direct action of external conditions, but its precise character is not."

I sought by these words to express the familiar fact that the stimulus under which a vital action takes place is one thing, while the character of the action itself is quite another thing.

This fact seems, from its very simplicity, to slip out of the minds of naturalists, and I should like to improve this opportunity to approach it from another standpoint.

We have been familiar for many years with two views of the nature of the process of development from the egg.

One school of embryologists holds that the organism arises from the egg by virtue of its inherent potency; that the constitution which the germinal matter has inherited is

in some way an embodiment of all that is to be unfolded out of it; while the other school finds, in the stimulus which one part of the segmenting egg or of the growing organism exerts on other parts, the explanation of each successive step in the process of development.

Advocates of these two views generally regard themselves as opponents, but is there any real antagonism?

We now have positive evidence enough for each view to convince me that both are true; that every change which takes place in the organism from the beginning of segmentation to the end of life is called forth by some external stimulus either within the body or without; and yet that the outcome of the whole process of development is what it is because it was all potential in the germ.

The gun does not go off until the cap explodes; but it hits the mark because it is aimed.

While the distinction between the stimulus to a vital change and the nature of the change itself is obvious enough in simple cases, we may easily become confused and lose sight of it in handling complicated problems.

A hen's egg does not develop without the stimulus of heat, but the view that heat causes the chick is too grotesque for a sane mind.

What interests us is not that it becomes a chick while a duck's egg in the same nest becomes a duckling, but that the one grows into exquisite adjustment to the life of fowls, while the other becomes as admirably adapted for the life of ducks.

Here the stimulus comes from the external world, but the case is just the same when it is internal.

The well-known results of castration prove that the normal development of male animals is dependent on some stimulus which comes to the parts of the growing body from the reproductive organs, but who can believe that this is an adequate explanation of the short, sharp horns, the thick neck and the ferocity of the bull, or the bright colors and high courage of the cock?

The only explanation of the origin of these useful structures worth considering is that which attributes them to the retention by the germ of the effects of past ages of selection.

We have no reason to take a different view when the result varies with the stimulus. Under one internal stimulus a bud becomes a jelly-fish, while under others it may become a hydranth, or a machopolyp or a blastostyle, but the problem we have to solve in this case as in others is the origin of a beautifully coördinated organism, with the distinctive characters of its species, and with exquisite fitness for a life like that of its ancestors.

I showed some years ago that a small crustacean, Alpheus heterochelis, develops from the egg according to one plan at Beaufort in North Carolina, according to a second at Key West in Florida, while it has still a third life history at Nassau in the Bahama Islands, but no one can believe that the influences which cause this diversity have anything to do with the final outcome of the process.

The case is exactly the same when a cell which normally gives rise to a half or a quarter of the body produces the whole under a different stimulus.

All the machinery in a great industrial exposition may be started by a single electric contact, but however much the discovery of the button may interest us, it helps us little to understand the result.

So it is with living organism. External conditions press the button, but it takes all the inherited potency of living matter to do the rest.

It is an error to believe that great knowledge is needful for a clear grasp of first principles. Too often a great store of information is like riches, "it cannot be spared nor left behind, but it hindereth the march;

yea, and the care of it sometimes loseth or disturbeth the victory."

Students who are drifting on the sea of facts with which the modern laboratory has flooded us declare that the doctrine of adaptation is antiquated and unscientific and pernicious.

They tell us organisms have many properties which are not adaptive, and that in many other cases we cannot tell whether a property is adaptive or not. Of course this is true. No one supposes that susceptibility to poisons, for example, is adaptive, and our knowledge of nature is incomplete beyond measure.

They tell us, too, that many attempts to explain the uses of parts are fanciful and worthless. Unfortunately, this is true also, but the logic which makes it a basis for denying the reality of adaptation is enough to call Paley from his grave.

While protoplasm is the physical basis of life, the intellectual basis of biology is adjustment.

I should like to see hung on the walls of every laboratory Herbert Spencer's definition to the effect that life is not protoplasm but adjustment, or the older teaching of the Father of Zoölogy that the essence of a living thing is not what it is made of nor what it does, but why it does it.

Spencer has given us diagrams to prove that the vertebral column has become segmented by the strain of flexion, but Aristotle tells us that Empedocles *and the ancients* are in error in their attempts to account for the jointing of the backbone by the strain of flexion, for the thing to explain, he says, is not how it becomes jointed, but how the jointed backbone has become so beautifully adjusted to the conditions of life.

"Is there anything of which it may be said: See, this is new. It hath been already in the old times which were before us."

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CURRENT NOTES ON ANTHROPOLOGY (III.).

THE EARLIEST ENGLISHMEN.

Some interesting studies as to the earliest signs of human industry in England deserve a notice.

The description by Professor Prestwich of some flint implements found by Mr. Harrison in pre-glacial strata on the chalk plateau of Kent seems to have added an impetus to such researches. Mr. O. A. Shrubsole describes a series of those relics from pre-glacial hill gravels in Berkshire, in the Journal of the Anthropological Institute for August, 1894; and in the May number of the same journal, Mr. A. M. Bell replies with considerable force to the objections which had been urged against Professor Prestwich's reasonings; vindicating for the Kent implements an antiquity beyond that of the formation of the present river valleys.

A pleasantly written volume on the subject is one by Mr. Worthington G. Smith entitled, *Man the Primeval Savage*. He discovered a true palaeolithic workshop, or rather several of them, in undisturbed relations, near Dunstable, about thirty miles north of London. The heaps of chips and broken flints lay just as the primeval artist had left them, covered to many feet in depth by the washings from the boulder clay. Mr Smith was able to collect the chips in a number of instances, and by fitting them together, reconstruct the original flint block from which the instrument had been formed; and then to make a cast of the size and shape of the tool represented by the cavity. This beautiful demonstration leaves nothing to be desired. He does not believe, however, that either his finds or those of the others mentioned are pre-glacial. His book is agreeably written and well illustrated. (Published by E. Stanford, London.)

THE TRIBES OF THE 'GRAN CHACO.'

The 'Gran Chaco,' or 'Great Hunting-ground,' merits its name, for it extends 850

miles in length by 350 in breadth, one vast forest and marsh, in the northern portion of the Argentine Republic. Much of it is unexplored and almost inaccessible. Its sparse human inhabitants are savage and wandering tribes, still in the stone age, shy and treacherous. Their linguistic classification presents extraordinary difficulties. Explorers have extended the same name to different stocks; and applied diverse names to the same stock.

An excellent monograph published in the Atti Della Societá Romana di Antropologia by Guido Boggiani is helpful as far as it goes. It is entitled '*I Ciamacoco.*' This is another form of *Zamuco*, the name of a tribe converted in the last century by the missionaries. But the modern is not a descendant of the ancient clan, scarcely any linguistic relative. The author presents an accurate vocabulary of about 250 words, and gives a full description of the primitive arts of the tribe, with 62 beautifully prepared illustrations. They still use the stone axe, the bow and arrow, feather and shell decorations, and other appurtenances of the pristine condition of culture.

Another band, the Chunupies, of the southern Chaco, is the subject of an article by J. B. Ambrosetti, in the Anales de la Sociedad Scientifica Argentina for 1894. He gives a short vocabulary and an ethnographic description.

Such work cannot be accomplished too soon, as these Chaco tribes are dying out with fearful rapidity, and probably half a century more will complete their extermination.

ARCHÆOLOGY AS A DEDUCTIVE SCIENCE.

WITHIN the last two years an interesting issue has arisen between two schools of archæologists, the one which *knows* just what man's early activities yielded, the other which prefers to learn about them by studying what relics can be found, and confining conclusions to their obvious teachings.

In America the former school is ably represented by Mr. W. H. Holmes and Mr. J. D. McGuire, of Washington. Mr. Holmes' lines of thought are fully set forth in the Proceedings of the Chicago Congress of Anthropology, in an article entitled *Natural History of Flaked Stone Implements.* He maintains that an implement is to be studied 'as the biologist studies the living creature;' and he therefore classifies such remains into 'species' and 'genera,' speaks of their 'lines of evolution,' and even of their 'ancestral forms,' and adds diagrams showing their genealogies.

Mr. McGuire, who has published several interesting articles on the methods of chipping and rubbing stone, in the *American Anthropologist*, has become so thoroughly master of the situation in that connection that he more than intimates that European archæologists have blundered in drawing a distinction between the 'rough stone age' and the 'polished stone age;' a position with which Mr. Holmes seems to sympathize. That neither of these learned writers has ever examined a European site, seems to them of light weight, as the 'natural history method' is sufficient.

Those of a different way of thinking have not been silent. In this country such students as Prof. Henry W. Haynes, of Boston, Mr. H. C. Mercer, of Philadelphia, and Mr. Thomas Wilson, of Washington, all of whom are personally familiar with the oldest 'stations' on both continents, have condemned as narrow and inapplicable the views of Messrs. Holmes and McGuire; and in the *American Naturalist*, for December, Mr. Charles S. Read, of the British Museum, in an exhaustive article, sets forth the uncertainties which must attend conclusions based on studies limited to one field of research. In the same tone are several articles in recent issues of *L' An-*

thropologie. Mr. McGuire returns to the charge in the January number of the *Naturalist*, but hardly strengthens his position. The discussion is not yet terminated. 'Replies' are announced; but at present, it must be said that the deductive and inferential method in archæology appears to be a dubious mode of procedure.

THE VANNIC LANGUAGE.

Most readers need not be told that the Vannic language means that which was once spoken in the region around Lake Van, in modern Armenia, by the people who called themselves Kaldi.

They came into contact with the Assyrians about 885 B. C., and adopted from them the cuneiform writing, by means of which they preserved their records in their own tongue. These have been zealously studied and collected of recent years, but without positive results. Professor Sayce maintains that the Vannic was a Georgian dialect, and has published from it various translations. Last summer, before the French Academy, M. Oppert pronounced all these translations illusory, denied that we know a single word of the tongue, and laughed at the names of the kings so seriously put forth by Sayce. The latter, however, in the *Journal of the Royal Asiatic Society* for October last, prints a bilingual inscription in good Assyrian and Vannic, where the texts correspond almost line for line, and claims in a number of examples to have proved by this confrontation the correctness of his earlier translations. He acknowledges that our defective acquaintance with the Assyrian is a difficult obstacle to a complete rendering.

The evidence that the Vannic was akin to the Georgian is, however, not increased by this bilingual text. It still remains more probable that it was either ancient Armenian, or some other long extinct Aryan dialect; possibly near to the Thracian, for which there is a little evidence in the similarity of proper names. The point is one of considerable ethnographic importance.

RECENT PUBLICATIONS ON CRANIOLOGY.

Two important contributions on the Craniology of the South American Indians have recently appeared.

The first is by Dr. Ten Kate on the skulls of the Araucanians of the Argentine Republic. His material was 119 crania in the Museum of La Plata (where his paper was published). He confirms the statement quoted in my *American Race*, p. 324, that these Indians are markedly brachycephalic, 96 out of the 119 having a cephalic index above 80. The proportion of artificially deformed specimens is large, numbering about 82 per cent. They present quite diverse varieties of deformation.

Two series from Southern Argentina, in the valley of the Rio Negro, are described with his customary minuteness by Dr. R. Virchow in the Proceedings of the Berlin Anthropological Society for 1894, pp. 386–408. One series was from the base of the Cordillera, and evidently was of Araucanian origin; the other, from near the Atlantic coast, presented marked dolichocephaly and probably came from Tzoneca burials. In this article Dr. Virchow incorporates some instructive observations on artificial cranial deformities in America generally, making a useful appendix to his remarks on that subject in his *Crania Ethnica Americana*.

The Smithsonian Miscellaneous Collections, No. 969, just issued, is a translation of *The Varieties of the Human Species* by Giuseppe Sergi, Professor of Anthropology in the University of Rome. His method of classification is based upon the theories of craniology of which he himself is the author. Instead of multiplying, *ad infinitum*, the measurements of the skull as so many craniologists affect, he classifies according to broad outlines of cranial shape,

believing that such are far more permanent and therefore more racial than the minor variations which have engaged the attention of others. His arguments are drawn from a conscientious study of ample series from various quarters of the globe, and though some of his refinements may not be sufficiently established, the general principles he advocates merit the careful consideration of cranial specialists, as containing some new and certainly correct observations. A short prefatory note by myself introduces the author to the American public.

THE ARYAN CRADLE-LAND.

IF anybody thinks that the question whether the primitive Aryan horde lived in Europe or Asia has been settled, he is mistaken. Two publications of late date show that the defenders of the old theory of their central Asian origin are nowise lacking in vigorous argument.

Prof. August Boltz, of Darmstadt, in a pamplet *Das Vedavolk in seinen Gesamtverhältnissen*, has worked out the problem of the origin and earliest migrations of the Aryans quite to his own satisfaction. He adds two maps, on which the reader can trace very clearly how they began in the great Tarim basin and about Lob Nor, and journeyed westward across the Pamir plateau, on the western slope of which they diverged, the Celtic stem wandering northwest into Europe north of the Black Sea; the Greek, Latin, Etruscan and Slavic branches by way of the Hellespont and the islands; the Iranian group remaining in Persia, while the Veda-folk or Indo-Aryans, ascended the mighty passes of the Hindu Kusch and Karakorum ranges to reach the fertile valleys to the south. These are pretty plans, but we look in vain for a substantial support to them.

Turning to Europe, M. De Nadaillac's admirable summary of the results of the investigations in the lake-dwelling of that continent (in a contribution to the *Revue des Questions Scientifiques* for October last, entitled *Les Populations Lacustres de l'Europe*) lifts the veil as far as at present possible on European culture in neolithic times—those times when the Aryan stock began its wide wanderings. The writer inclines to their Asian origin; but with his customary frankness he acknowledges that nowhere in the debris of these ancient dwellings has there a single positive sign of Asiatic art been discovered, nor any relic such as we might suppose even a savage tribe would carry from its pristine home. Until down to a late period of prehistoric time, European culture seems to have been indigenous. For a clear and accurate summary of what it was among the lake-dwellers, the student would do well to peruse the article referred to.

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*TCHÉBYCHEV.**

OF Russian mathematicians, second only to Lobachévsky should be ranked Pafnutij Lvovitsch Tchébychev.

Born in Russia in 1821 and formerly professor at the University at St. Petersburg, he reached deservedly the very highest scientific honors, being privy councillor, the representative of applied mathematics in the Imperial Academy of St. Petersburg, in 1860 made member of the famous Section I.–Géométrie, of the French Académie des Sciences, and afterward *Associé étranger*, the highest honor attainable by a foreigner.

His best known work is the justly celebrated *Mémoire sur les nombres premiers*, Académie Impériale de Saint Pétersbourg, (1850), where he established the existence of limits within which the sum of the logarithms of the primes inferior to a given number must be comprised. This memoir is given in *Liouville's Journal*, 1852, pp. 366–390.

* Deceased December 8, 1894.

Sylvester afterward contracted Tchébychev's limits; but the original paper remains highly remarkable, especially as it depends on very elementary considerations.

In this respect it is in striking contrast to the equally marvelous paper of the lamented Riemann, *Ueber die Anzahl der Primzahlen unter einer Gegegebenen Grösse* presented to the Berlin *Academia* in 1859. Tchébychev had in 1848 presented a paper with this very title to the St. Petersburg Academie; *Sur la totalité des nombres premiers inferieurs á une limite doninée.* (Given in Liouville's Journal, 1852, pp. 341–365.)

Riemann speaks of the interest long bestowed on this subject by Gauss and Dirichlet, but makes no mention of Tchébychev. However, Sylvester speaks of 'his usual success in overcoming difficulties insuperable to the rest of the world.'

But though best known for his work in the most abstract part of mathematics, in reality Tchébychev was of an eminently practical turn of mind.

Thus it was his work, *Theorie des mechanismes connus sous le nom de parallélogrammes* (Mémoirs des savants étrangers, Tom. VII.), which led him to the elaborate dissertation *Sur les questions de minima qui se rattachent a la représentation approximate des fonctions*, 91 quarto pages in Mémoirs de l' Académie Impériale des Sciences de Saint Pétersbourg, 1858. While the variable x remains in the vicinity of one same value we can represent with the greatest possible approximation any function $f(x)$, of given form, by the principles of the differential calculus. But this is not the case if the variable x is only required to remain within limits more or less extended. The essentially different methods demanded by this case, which is just the one met in practice, are developed in this memoir.

The same line of thought led to his connection with a subject which has since found a place even in elementary text-books, namely rectilineal motion by linkage.

He invented a three-bar linkage, which is called Tchébychev's parallel motion, and gives an extraordinarily close approximation to exact rectilineal motion; so much so that in a piece of apparatus exhibited by him in the London Loan Collection of Scientific Apparatus, a plane supported on a combination of two of his parallel motion linkages seemed to have a strictly horizontal movement, though its variation was double that of the tracer in the simple parallel motion.

Tchébychev long occupied himself with attempting to solve the problem of producing exact rectilineal motion by linkage, until he became convinced that it was impossible and even strove long to find a proof of that impossibility. What must have been his astonishment then, when a freshman student of his own class, named Lipkin, showed him the long sought conversion of circular into straight motion. Tchébychev brought Lipkin's name before the Russian government, and secured for him a substantial reward for his supposed original discovery.

And perhaps it was independent, but it had been found several years previously by a French lieutenant of engineers, Peaucellier, and first published by him in the form of a question in the *Annales de Mathematique* in 1864. When Tchébychev was on a visit to London, Sylvester inquired after the progress of his proof of the impossibility of exact parallel motion, when the Russian announced its double discovery and made a drawing of the cell and mounting. This Sylvester happened to show to Manuel Garcia, inventor of the laryngoscope, and the next day received from him a model constructed of pieces of wood fastened with nails as pivots, which, rough as it was, worked perfectly. Sylvester exhibited this to the Philosophical Club of the Royal So-

ciety and in the Athenæum Club, where it delighted Sir Wm. Thomson, now Lord Kelvin, and led to the extraordinary lecture *On Recent Discoveries in Mechanical Conversion of Motion*, delivered by Sylvester before the Royal Institution on January 23, 1874. This in turn led to Kempe's remarkable development of the subject, and to Hart's discovery of a five-bar linkage which does the same work as Peaucellier's of seven.

Henceforth Peaucellier's Cell and Hart's Contraparallelogram will take their place in our text-books of geometry, and straight lines can be drawn without begging the question by assuming first a straight edge or ruler as does Euclid.

Thus Kempe's charming book, '*How to Draw a Straight Line*,' is a direct outcome of Tchébychev's sketch for Sylvester. As might perhaps have been expected, the immortal Lobachévsky found in his compatriot a devoted admirer. Not only was Tchébychev an active member of the committee of the Lobachévsky fund, but he took the deepest interest in all connected with the spread of the profound ideas typified in the non-Euclidean geometry. Knowing this, Vasiliev in his last letter asked that a copy of my translation of his address on Lobachévsky be forwarded to the great man. His active participation in scientific assemblies is also worthy of note; for example, at the 'Congrès de l' association française pour l'avancement des sciences, à Lyon,' he read two interesting papers, *Sur les valeurs limites des intégrales*, and *Sur les quadratures*, afterwards published in *Liouville's Journal*.

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SCIENTIFIC LITERATURE.

Les Oscillations Électriques. H. POINCARÉ. (CONCLUDED.)

Propagation of Electrical Oscillations Through Air.—The velocity of propagation of electromagnetic induction through dielectrics offered the first experimental test of superiority of the Faraday-Maxwell theory over the older theories. According to these that velocity should be infinite; according to the Faraday-Maxwell view of electromagnetic phenomena it should be the same as that of light. Poincaré reviews carefully all the experimental evidences bearing upon this point. Hertz's experiments in Carlsruhe are first discussed and his early failures in arriving at a satisfactory result are pointed out. Two methods employed in these measurements by Hertz at Carlsruhe and at Bonn are described briefly. One of these consisted in measuring by means of a resonator the difference of phase between two waves sent forth by the same oscillator, one wave along a conducting wire and the other through the dielectric in the vicinity of the wire. The other method consisted in measuring what Hertz considered the wave length of stationary electric waves in air formed by the interference between the direct waves sent forth by an oscillator and the waves reflected by a large flat mirror consisting of a metal sheet 2 meters wide and 4 meters high. In all these experiments the velocity of propagation along the wire seemed to come out considerably different from and generally less than that in air. But the methods were open to several criticisms. In the first place, the hall in which these experiments were carried out was too small for the wave lengths employed; secondly, the influence of the waves reflected from the walls was entirely neglected; thirdly, the dimensions of the reflecting mirror were not large enough in comparison to the wave length to prevent errors of observation due to the misleading influence of diffraction phenomena. All these objections were in a measure overcome in the earliest experiments of Sarasin and de la Rive (C. R. t. CX. p. 72). In these experiments the methods of Hertz were employed, but they were performed in a large hall, with a large

mirror and with smaller resonators. The results improved with the increase of the dimensions of the mirror and the diminution of the size of the exploring resonators. In a subsequent series of experiments (C. R. CXX., p. 688) carried out in a very large hall with a mirror 8 meters high and 16 meters wide and employing circular resonators of 50 and 75 centimeters in diameter these investigators obtained completely satisfactory results, proving beyond all reasonable doubt that the velocity of propagation of electromagnetic waves through dielectrics is the same as along conducting wires and equal to the velocity of light. The sources of error in Hertz's experiments were clearly demonstrated by these experiments, for no matter how large were the hall and the mirror a sufficient increase in the dimensions of the exploring resonators would always give misleading results, similar to those obtained by Hertz.

But among the many encouraging results obtained by Sarasin and de la Rive there is one result which causes much anxiety to the mathematical physicist. It is the serious disagreement between the theoretically calculated period of the resonator and that determined experimentally by the illustrious physicists of Geneva. In an exceedingly interesting mathematical discussion of the functions of the resonator Poincaré shows that the wave length of the fundamental vibration can differ but little from twice the circumference of the resonator, whereas Sarasin and de la Rive found it to be equal to eight times the diameter. The cause of this disagreement must be explained by the theory, but how? Poincaré gives no definite answer to this question. Many valuable suggestions are thrown out, however, and the subject is then dismissed after showing by a reference to Blondlot's and Bjerkness' experiments that the theory of the resonator just given is correct in its main features. No other theory of the resonator has been given since that given by Hertz, and Poincaré's discussion contains many valuable additions to the rough outline of the subject sketched out by Hertz. In this connection the reviewer ventures to refer to a paper by Professor P. Drude (*Zum Studium des Electrischen Resonators*, Wied. Ann. Nov. 1894).

Reflection and Absorption of Hertzian Waves.—Resonator and mirror form the essential instruments in every method of studying electrical waves in the dielectric. The phenomena of reflection and absorption of these waves deserve, therefore, careful analysis. To these Poincaré devotes his attention now. The case of orthogonal incidence upon a plane metal mirror is first discussed. It is shown that the penetration of the wave into the metal is inversely proportional to the square root of the product of conductivity and permeability of the metal and directly proportional to the square root of the wave length. For instance, a wave of a periodicity of 50 millions per second, which is the ordinary Hertzian frequency, will be reduced to nearly one-third of its initial intensity at a distance of $\frac{1}{50}$ mm. below the surface of a mirror of copper. The relation, however, which Poincaré obtains between the penetrability of the wave and the wave length, the conductivity, permeability, and specific inductive capacity of the metal does not hold good for frequencies as high as those of light, for on the one hand it gives by approximation a negative value for the specific inductive capacity of all metals, and on the other hand it gives a conductivity 300 to 400 times smaller than that obtained by ordinary resistance measurements. The same relations hold good for oblique reflection. It is interesting to note that if, as Cauchy believes, the fundamental equations of Fresnel (slightly modified) hold good for metallic reflection then a retardation in phase equal to half a period takes place at

the reflecting surface when the electric force of the incident wave is normal to the plane of incidence; no retardation takes place if this electrical force is in the plane of incidence. The extinction of the wave in its passage through the metal develops heat and Poincaré calculates the rate at which the heat is developed by a given current, obtaining the interesting result that it is proportional to the square root of the product of frequency, specific resistance and permeability. The results of these considerations are now compared to experiment. The most important experiments bearing upon this part of the theory are those of Bjerkness (l. c.). A circular resonator having a small plate condenser interposed in place of the spark gap was employed. Between these plates a small aluminum sheet was suspended and measured by its deflection the mean square of the potential difference between the plates. The oscillator was gradually tuned and the resonance effect in the resonator measured by the deflection of the aluminum sheet. Six resonators of the same dimensions but of different material were investigated. The resonance curve of copper was highest, then followed brass, silver, platinum, nickel and iron, in the same order as required by theory. The resonator decrement of iron, for instance, was nine times and that of platinum twice as large as that of copper. To measure the depth of penetration these materials were deposited electrolytically, say iron on a copper resonator, or *vice versa*, and the resonator effect measured for the various thicknesses of the deposit. Results agreeing very fairly with the theory were obtained.

Propagation of Electrical Waves through Dielectrics other than Air.—Another crucial test of the correctness of the Faraday-Maxwell theory is furnished by the well known relation that the specific inductive capacity of a dielectric is equal to the square of its index of refraction. This relation is an immediate inference from the new electromagnetic theory. Since the index of refraction of a substance is equal to the ratio of the velocity of propagation in vacuum to that in the substance it follows that the velocity of propagation of a Hertzian wave in dielectrics having a specific inductive capacity larger than unity should be smaller than in air. This relation was tested by Blondlot in the experiments cited above by immersing both the conducting wire and the resonator in a liquid dielectric and measuring the wave length. Another method based upon the same principle was that employed by Rubens & Arons (Wied. Ann. 40 p. 585). The neutral point of a rectangular resonator was connected directly to one side of the spark-gap of the oscillator. No spark was then observed in the spark-gap of the resonator. If, however, the balance of the resonator was now disturbed by inserting on one side of it a certain length of wire immersed in a dielectric the spark appeared. The balance was again restored by inserting a sufficient length of wire in the other side of the resonator. The ratio of these two lengths of wire measured the ratio of the velocities of propagation in air and in the dielectric.

Another method, first employed by J. J. Thomson (Phil. Mag. 30, p. 129), was based on the relation which exists between the capacity of a plate condenser and the dielectric constant of the insulator separating its plates. The period of an oscillator or resonator will vary with the dielectric between the condenser plates. Thomson measured the period of an oscillator for various dielectrics placed between its condenser plates and calculated from it the specific inductive capacity. Several other electromagnetic methods are described briefly by Poincaré, and then the statical methods, belonging most of them to the pre-Hertzian epoch, are passed in quick review. Finally the experimental results are coördinated

and briefly discussed. In a large number of cases Maxwell's relation is confirmed; but, again, the cases are numerous in which the agreement between theory and experiment is far from satisfactory; this is especially true of dielectrics showing traces of conductivity and large electric absorption, and even more true of electrolytes. This part of Poincaré's work is rather incomplete, probably because it offers fewer opportunities to a mathematical physicist than any other part of Maxwell's electromagnetic theory. The most serious criticism, perhaps, that may be brought against it is its omission of some of the most important investigations on dielectric constants, as, for instance, the investigations of Boltzmann. Again, not a single word is said concerning the influence which the study of the dielectric properties of substances had upon Faraday and Maxwell and how much it had contributed to the formation of their electromagnetic theory.

The reflection of electrical waves from the surface of a dielectric is taken up and it is shown by a reference to analogous phenomena in optics why reflection cannot occur when the thickness of a dielectric plate is small in comparison to the wave length of an electrical wave. Trouton's experiments (Nature, Vol. 39, p. 391) form the basis of this discussion.

The experimental evidence furnished by the study of the reflection of electrical waves is cited which supports the view that the plane of polarization as defined in optics is perpendicular to the direction of the electrical force in the wave-front.

A very interesting experimental investigation published by Klemencic (Wiener Sitzungsber, 19. Feb., 1891) is next described. It treats of wave reflection by dielectrics. The dielectric experimented with was a slab of sulphur 120 cm. long, 80 cm. wide and 7 cm. thick. The wave length employed was 60 cm. A rectilinear oscillator placed in the axis of a cylindrical parabolic mirror furnished the plane waves. The reflected and refracted waves were studied by means of thermoelectric couples attached to rectilinear oscillators placed in the axis of parabolic mirrors similar to the one used in connection with oscillator. There was a reflection at every angle of incidence when the direction of oscillation of the electrical force was perpendicular to the plane of incidence. But when it was parallel to it then there was an angle of incidence at which no reflection occurred. Fresnel's fundamental formulæ, however, were not quite satisfactorily verified. Poincaré ascribes it to the insufficient thickness of the slab. Klemencic found also that the energy of the incident wave was smaller than the sum of the energies of the reflected and refracted wave, a result which he believed to be due to the presence of diffraction.

Conductors in Motion in an Electromagnetic Field.—The last chapter gives the essential features of Hertz's essay: On the fundamental equations of the electromagnetic field for conductors in motion.

Poincaré considers first the *electromotive force* induced in a circuit which is moving through a variable electromagnetic field. He proceeds as follows: Consider a surface formed by the circuit under consideration. Let it move with the circuit. Consider two consecutive positions of this surface, the time of passage from the first to the second position being infinitely short, the velocity of motion being finite. Consider now the space bounded by the initial and the final position of the surface and by the ring-shaped surface whose boundary is the initial and the final position of the circuit. The total magnetic flux through this surface is according to well known relations proportional to the total amount of what Hertz and Poincaré call *true magnetism* included in the bounded surface. The total induced electromotive force being equal to the total

rate of variation of the magnetic flux through the circuit the last relation leads to the following final result: The total electromotive force induced in an infinitely small circuit which moves through a variable electromagnetic field is composed of three parts. First, the electromotive force due to rate or variation of the magnetic flux through the circuit and produced by the time variation of the field itself. Second, the electromotive force due to the rate of variation of the magnetic flux through the circuit produced by the motion of the circuit. The third component of the induced electromotive force can be described as follows: Suppose that permanent magnetic charges are distributed in any way whatsoever throughout the field. There is then a transference of magnetic matter through the moving circuit. We may call it the magnetic convection current, following a suggestion of Hertz (Unters. ueb. d. Ausbr. der el. Kraft, p. 265). This magnetic convection current is equal to the quantity of magnetic matter contained in the volume traced out per unit of time by the moving circuit, and is proportional to the third component of the induced electromotive force. This component does not appear in Maxwell's theory, so that the Hertzian equations seem to be more complete than those of Maxwell.

Poincaré recognizes in this quite a difference between Maxwell's presentation of the electromagnetic theory and that of Hertz; but this difference will evidently exist only if it is proved that a distribution of permanent magnetism, whose induction flux over a closed surface is a constant, different from zero, can exist. The physical meaning of such a distribution is far from being clear, and Poincaré might have well devoted more attention to the elucidation of this perplexing feature of the Hertzian equations. On this point the student will do well to consult Boltzmann (Vorles. über Maxwell's Theorie d. Elec. & d. Lichtes, II. Theil, IX. Vorles.).

The second group of equations refers to the magnetomotive force induced in a circuit which is changing its position with respect to a field of given distribution of electrical force and it is shown that the total magnetomotive force induced in an infinitely small circuit in motion is composed of four components. The first component is proportional to the rate of change of the flux of electric induction which constitutes the conduction current. The second component is proportional to the rate of change of the flux of electric induction which constitutes the displacement current. The third component is proportional to the rate of change of the electric flux due to the motion of the circuit, and the fourth component is proportional to the convection current of permanent electrostatic charges, corresponding to what was called above the convection current of permanent magnetism. There is, however, no difficulty of conceiving a permanent electrification of the dielectric such that the total flux of its induction through a closed surface should be different from zero, and, therefore, the magnetomotive force induced by an electrical convection current is *a priori* evident as soon as the correctness of the fundamental assumptions in the Faraday-Maxwell theory is admitted. There is no difference between this second group of equations and those given by Maxwell.

It is pointed out that the existence of the third component was verified by Rowland's experiments (Pogg. Ann. 158, p. 487), and the existence of the fourth component by the experiments of Roentgen (Wied. Ann. 35, p. 264). The magnetomotive force due to displacement currents was, of course, first pointed out by the experiments of Hertz.

Next follows a beautiful mathematical discussion of the mechanical forces acting upon a body which is moving through an electromagnetic field. The following types

of forces are passed in quick review: 1. An ordinary magnetic force due to the presence of permanent magnetism. 2. Ordinary electrostatic force due to the presence of electrostatic charges. 3. Electromagnetic force consisting of four distinct components. One component is the electromagnetic action of the field upon conduction currents. The second component is the electromagnetic action of the field upon the displacement currents. The third component corresponds to the electromagnetic action of the field upon the currents observed by Rowland and Roentgen. The fourth type of force is that between a variable current and the electrical reactions set up in the field by its variation. All these forces except the last have been observed experimentally. The last one is too feeble to be detected by any of the known experimental methods.

The work is, unfortunately, marred by quite a number of typographical errors. Some of them occur in the midst of important and rather difficult mathematical operations and will undoubtedly be a source of considerable perplexity to the younger students for whom, especially, this work is intended.

The reviewer is of the opinion that he will reëcho the sentiment of every lover of the Faraday-Maxwell electromagnetic theory when he states that this, the latest, contribution of the brilliant French mathematician will be a welcome guide to everyone who wishes to keep in close contact with the latest advances of the electromagnetic theory.

M. I. Pupin.

Columbia College.

The Steam Engine and Other Heat Engines. By J. A. Ewing, Professor of Mechanism and Applied Mechanics in the University of Cambridge. Cambridge University Press; New York, Macmillan & Co. 1894. 8vo., pp. xiv + 400. Price, $3.75.

Professor Ewing, in his article on the steam engine in the *Encyclopædia Britannica*, gave good measure to his ability and knowledge of the subject by the production of a treatise in which, for the first time, a systematic and fairly complete discussion was attempted of the theory of the real steam engine, as distinguished from the purely Thermodynamic Theory of the Ideal Heat Engine, which only had previously been presented by writers on that wonderful machine. Clark and Hirn and Iserwood had cleverly shown the wide discrepancy between the ideal and the real engine, and Cotterill had discussed with elegance and clearness the extra thermodynamic losses of the machine; but Ewing brought together, for the first time, and in such form as to make his discussion useful, to theorist and 'practical man' and professional engineer alike, in the study of existing engines and in the attempt to improve upon them by scientifically accurate designing and construction. His article was a condensed, but complete, exposition to its date, of scientific and practical knowledge of the methods of economical production of heat in the boiler, and of the economical thermodynamic utilization of the energy thus made available at the engine, with exact accounts of the various methods of waste of thermal and of dynamic energy. Had its author written nothing else, this article would have sufficed to give him a full share of fame.

His new treatise on the steam engine, now issued in book form, is based upon his earlier discussion, but is entirely rewritten to give it a shape better adapted to its present purpose, and to permit the introduction of new matter. "The endeavor has been, throughout, to make evident the bearing of theory on practical issues." Some space is devoted to experimental work and the discussion of facts and data revealed by it. In so condensed a work it would have been impossible to introduce as complete a study of pure thermodynamics as may be found in

Wood or Peabody, as full treatment of the extra-thermodynamic wastes as in Cotterill, or of experimental methods as in Carpenter; but the book exhibits much of that rarest of talents, ability to condense, and, for an abridged work, maintains an extraordinarily high standard of scientific quality. The discussion of the 'entropy-temperature' diagram of Professor J. Willard Gibbs, which is only now, after many years, finding its place in the treatment of the heat motors, is the fullest and most satisfactory yet produced, not even excepting the work of its first trans-Atlantic advocate, Mr. J. Macfarlane Gray. This method of graphical treatment is gradually finding its place, and a very useful one, in the discussion of thermodynamic machines. Following Wood and Peabody, and later writers, this author has adopted, in all his own computations, the value, 778, for the thermodynamic equivalent obtained by Rowland. It may probably be safely asserted that this value is now universally accepted.

The unavoidable brevity with which all topics are treated in so small a space gives the reader occasion, frequently, to wish that the volume had been doubled in size, and fuller discussion and more of result thus secured; but the book takes its place, among the many other treatises on the steam engine, as meeting a need that is being continually felt more and more by engineers, and which is not as well supplied by any other of the existing abridged discussions of the theory of the machine. It is well up to date in its practical aspects, as well as in the van on its purely scientific side.

R. H. THURSTON.

CORNELL UNIVERSITY.

An Introduction to Chemical Analysis for Beginners.—From the Sixth German Edition of DR. FR. RUDORFF.—Translated by CHAS. B. GIBSON and F. MENZEL.—Chicago, The W. J. Keener Co. 8 vo., 96 pp. Price $1.00

This book is divided into two parts: Part I, Reactions; and Part II, Systematic Course of Qualitative Analysis. Metallic copper is the first substance examined, and then follow copper, zinc, zinc chloride, manganous sulphate, iron, lead, etc., in the order named. A careful examination of this part fails to detect any great novelty either of matter or arrangement. In Part II the metals are grouped under the familiar group reagents except that lead, mercury and silver are placed along with those precipitated by hydrogen sulfid and not, as is usual, separated under hydrochloric acid as group reagent. The scheme of analysis is well conceived, but offers little of novelty. The explanations and notes have been carefully adjusted to meet the needs of the student and are a valuable feature. The translation is, however, a very slovenly piece of work, and the nomenclature is especially bad. For example, on page 72, we find 'ammonic' sulfid written Am_2S, and lower down we have NH_4OH. Why the authors deny to bismuth cobalt and nickel the ic terminations which they give to nearly all the other metallic salts is not apparent. Several very awkward sentences occur. For example, in the introduction, "We have made a few additions calculated to assist the medical and dental student who suffers mainly the disadvantage of being unable to devote but a small part of his time to chemical studies."

The mechanical execution of the book is pretty good. There is no index.

EDWARD HART.

LAFAYETTE COLLEGE.

NOTES AND NEWS.

PALEOBOTANY.

A LARGE collection of fossil plants made by Professor W. P. Jenny in the Cretaceous rim of the Black Hills during the past field season has just been opened at the National Museum and proves to be of the highest interest to paleontology. It was made under

unusual difficulties and in the pure love of science in connection with Professor Jenney's work as a mining expert in the Black Hills. All the material comes from the lower portion of what was regarded by Professor Newton as the Dakota group; most of it from nearly the same horizon as that from which the gigantic cycadean trunks now so well known and the small collection of plants made by Jenney and Ward in September, 1893, were obtained (see *Journal of Geology* for April–May, 1894, Vol. II., No. 3, pp. 250–266). The collection has not yet been systematically worked up, but a casual examination of it shows that the plants have no relation to the true Dakota group, but are certainly as old as Lower Cretaceous and are probably of Kootanie age. The genera *Gleichenia*, *Cladophlebis*, *Zamites*, *Athrotaxopsis*, and many others characteristic of the Kootanie, the Trinity and the Potomac formations are represented, while no dicotyledonous leaves occur. Upon the whole they may be considered as a complete confirmation of the conclusion previously reached that the Dakota group of Newton must be subdivided and that a large portion of it belongs to the Lower Cretaceous. Professor Jenney is able to separate it into five distinct horizons, only the uppermost of which belongs to the Dakota of Meek and Hayden, between which and the underlying beds he finds an unconformity.

Mr. Lester F. Ward delivered two lectures on Jan. 8 and 10 before the Peabody Institute of Baltimore, on the *Vegetation of the Ancient World*, illustrated by over fifty lantern views. These were arranged in such a manner as to pass in review in their ascending geological order all the fossil floras known from the Silurian to the Pleistocene. The greater part of the illustrations were drawn from American material, and all the great plant bearing horizons of North America were represented by groups of typical and characteristic forms. Special attention was given to the wonderful fossil forests of this country, and especially of the National Yellowstone Park. The fossil flora of the Potomac formation, and particularly that of the State of Maryland and the City of Baltimore, were duly emphasized. Interspersed with these more scientific illustrations there were thrown on the screen a number of the magnificient ideal landscapes conceived and executed by the great scientific artists, Unger, Heer, Saporta and Dawson. The lectures were well adapted to give to the general public a systematic and comprehensive view of the forms of plant life that have inhabited the earth and especially those that have flourished in America throughout the past ages of geological time.

A TOPOGRAPHICAL ATLAS.

The Director of the United States Geological Survey has recently submitted to the Secretary of the Interior an amendment to the 'Sundry Civil Bill,' now before Congress, authorizing the printing and distribution of an atlas of ten topographical mapsheets to the schools, academies and colleges of the country, the proposed atlas to contain illustrations of the various types of topographical form observed in the country, and to be accompanied by an explanatory bulletin which will serve as a primer of topography for school use.

If the amendment is carried, and the atlas meets the approval of teachers, it is proposed to distribute additional series in later years. Those who are interested in the advance of geography in the schools cannot do better than promptly to address their Congressman, asking for support of this excellent proposition. It is in effect an economical measure, for it will at a moderate cost give a wide and novel use to a large amount of material that has been gathered at great expense, and that is now stored

in the office of the Geological Survey, awaiting a limited distribution some years hence.

BIBLIOGRAPHY OF AMERICAN BOTANY.

THE Bibliography Committee of American botanists has just completed its first year of organized work in the production of an author catalogue of papers relating to American Botany. This has been printed in the monthly issues of the *Bulletin of the Torrey Botanical Club* and then reprinted on library cards by the Cambridge Botanical Supply Co. The editors have endeavored to make the record as complete as possible and it includes 575 titles. The committee and the editors earnestly request that their attention be called to omissions and that all interested aid in insuring completeness.

Foreign botanists are particularly requested to call our attention to any of their writings which refer to American plants. Communications may be addressed to the Editor of the Torrey Botanical Club, Columbia College, New York City.

GENERAL.

ON January 10th, Dr. George M. Dawson, C. M. G., F. R. S., was appointed Director of the Geological Survey of Canada, succeeding Dr. Selwyn, retired.

THE next annual meeting of the British Association for the Advancement of Science will be held at Ipswich, commencing on Wednesday, September 11th. Sir Douglas Galton is President-elect.

ACCORDING to the daily papers a party composed of Prof. Charles E. Hite, Alfred C. Harrison, Jr., Henry C. Walsh and Dr. J. Donnell McDonald sailed on Wednesday to Central America with a view to obtaining natural history and archæological collections. The expedition is under the auspices of the biological department of the University of Pennsylvania.

SCIENTIFIC JOURNALS.

AMERICAN CHEMICAL JOURNAL, JAN.

Contributions from the Laboratory of General Chemistry, University of Michigan:—(1) *On the Action of Chlorcarbonic Ester on Sodium Acetone*: By PAUL C. FREER. (2) *The Action of Metals on Nitric Acid:* By GEORGE O. HIGLEY. (3) *An Introductory Study of the Influence of the Substitution of Halogens in Acids, upon the Rate and Limit of Esterification:* By D. M. LICHTY. (4) *On the Action of Sodium on the Esters of Aconitic and Citric Acids.* Preliminary Notice, by PAUL C. FREER.

The Combination of Sulphur with Iodine: By C. E. LINEBARGER.

Contributions from the Chemical Laboratories of the Massachusetts Institute of Technology:—*An Investigation of the Twitchell Method for the Determination of Rosin in Soap:* By THOMAS EVANS and I. E. BEACH.

A Laboratory Method for the Preparation of Potassium Fericyanide: By M. S. WALKER.

Reviews.

THE PHYSICAL REVIEWS, JAN.–FEB.

The Apparent Forces between Fine Solid Particles Totally Immersed in Liquids—I: W. J. A. BLISS.

The Distribution of Energy in the Spectrum of the Glow-lamp: EDWARD L. NICHOLS.

The Influence of Heat and the Electric Current upon Young's Modulus for a Piano Wire: MARY C. NOYES.

Minor Contributions: (1) *On Magnetic Potential:* FREDERICK BEDELL. (2) *A Method for the Study of Transmission Spectra in the Ultra-violet:* ERNEST NICHOLS. (3) *The Photography of Manometric Flames:* WILLIAM HALLOCK.

THE AMERICAN NATURALIST, JAN.

Birds of New Guinea: GEORGE S. MEAD.

Leuciscus Balteatus (Richardson), A Study in Variation: CARL H. EIGENMANN.

NEW BOOKS.

A Text-book of Organic Chemistry. A. Bernthsen. Translated by George McGowan. London, Blackie & Sons; New York, D. Van Nostrand. 1894. Pp. x ix+596. $2.50.

A Text-book of Mechanics and Hydrostatics. Herbert Hancock. New York, D. Van Nostrand. 1894. Pp. v + 408. $1.75.

A Treatise of Industrial Photometry with Special Application to Electric Lighting. A. Palaz. Translated from the French by George W. Patterson, Jr., and Merib Rowley Patterson. New York, D. Van Nostrand; London, Sampson Low, Marston & Co. Limited. 1894. Pp. vii + 322. $4.00.

Proceedings of the International Electrical Congress held in the City of Chicago, August 21st to 25th, 1893. New York, American Institute of Electrical Engineers. 1894. Pp. xxiv + 487.

The Life and Writings of Rafinesque. Richard Ellsworth Call. Louisville, Ky., Filson Club Publications, X. Quarto, pp. xii + 227.

History of Higher Education in Rhode Island. William Stowe Tolman. Washington, Government Printing Office. 1894. Pp. 210.

The Birds of Eastern Pennsylvania and New Jersey. Witmer Stone. Philadelphia, Delaware Valley Ornithological Club. 1894. Pp. vi + 185.

An Illustrated Dictionary of Medicine, Biology and Allied Sciences. George M. Gould. Philadelphia, P. Blackiston & Sons. 1894. xv + 1633.

Municipal Government in Great Britain. Albert Shaw. New York, The Century Co. Pp. 385. $2.

Eine Discussion der Kräfte der Chemischen Dynamik. Ludwig Stettenheimer. Frankfort, H. Bechhold. 1895. Pp. 85. M. 6.

On the Origin of Language and The Logos Theory. Ludwig Noiré. Chicago, Open Court Publishing Co. 1895. Pp. 57. 15 cents.

Geological Survey of Alabama. Eugene Allen Smith. Montgomery, Alabama, The Brown Printing Co. 1894. xxiv + 759; also Geological Map of Alabama.

Freytag's Technique of the Drama. Translated by Elias J. MacEwan. Chicago, S. C. Griggs & Co. 1895. Pp. ix + 366.

Social Growth and Stability. D. Ostrander. Chicago, S. C. Griggs & Co. 1895. Pp. 191. $1.